Effective Macroprudential Policy

Cizel, Janko; Frost, Jon; Houben, Aerdt; Wierts, Peter

published in
Journal of Money, Credit and Banking
2019

DOI (link to publisher)
10.1111/jmcb.12630

document version
Publisher's PDF, also known as Version of record

document license
Article 25fa Dutch Copyright Act

citation for published version (APA)
Cizel, J., Frost, J., Houben, A., & Wierts, P. (2019). Effective Macroprudential Policy: Cross-Sector Substitution from Price and Quantity Measures. *Journal of Money, Credit and Banking*, *51*(5), 1209-1235.
https://doi.org/10.1111/jmcb.12630

DOI: 10.1111/jmcb.12630

JANKO CIZEL
JON FROST
AERDT HOUBEN
PETER WIERTS

Effective Macroprudential Policy: Cross-Sector Substitution from Price and Quantity Measures

Macroprudential policy is increasingly being implemented worldwide, and is mostly applied to banks. A key question is whether this prompts substitution toward nonbank credit. Using two different global data sets on macroprudential measures and different methodologies, including detrended series, panel estimations, and propensity score matching, we find evidence of such substitution. Substitution toward nonbank credit appears to be stronger when policy measures are binding and are implemented in economies with well-developed nonbank credit markets. This substitution partially offsets the fall in bank credit, thus dampening the policies' effect on total credit.

JEL codes: E58, G10, G18, G20, G58
Keywords: financial cycle, macroprudential regulation, financial supervision, (shadow) banking.

Macroprudential policy is alive and kicking. It is being used actively both in emerging market economies and—following the financial crisis—in advanced economies.[1] This includes measures that apply directly to lenders, such

For helpful comments, the authors thank two anonymous referees, the editor Bob DeYoung, as well as Erlend Nier, Stijn Claessens, Jakob de Haan, Dirk Schoenmaker, Niklas Nordman, and Claudia Buch. We also thank participants at a workshop of the BIS Committee on the Global Financial System (CGFS), a meeting of the ESRB Instruments Working Group (IWG), an ECB-IMF conference, and an IMF MCM Policy Forum. We thank Nander de Vette for coding support, and Netty Rahajaan, Sonia Echeverri, Adriana Rota and Girlee Salmingo for help with typesetting and proofs. The views expressed in this paper are those of the authors and do not necessarily reflect the position of De Nederlandsche Bank, the Financial Stability Board or any other organizations that the authors may be affiliated with.

Janko Cizel *is reachable at* jcizel@gmail.com. Jon Frost *is at De Nederlandsche Bank, Financial Stability Board, and Cambridge Centre for Science and Policy* (*E-mail*: jon.frost@bis.org). Aerdt Houben *is at De Nederlandsche Bank and University of Amsterdam* (*E-mail*: a.c.f.j.houben@dnb.nl). Peter Wierts *is at De Nederlandsche Bank and VU University* (*E-mail*: p.j.wierts@dnb.nl).

Received October 9, 2015; and accepted in revised form November 13, 2018.

1. In the EU, no less than 47 substantive macroprudential measures were taken in 2014, and a further 60 in 2015 (ESRB 2016). Most EU Member States took macroprudential action in 2016 and 2017 (ESRB 2018).

Journal of Money, Credit and Banking, Vol. 51, No. 5 (August 2019)

as countercyclical capital buffers or capital surcharges, and restrictions that apply to borrowers, such as loan-to-value (LTV) and debt-to-income (DTI) ratio caps. Most macroprudential measures implemented around the globe between 2000 and 2013 apply to the banking sector only, including the borrower-based measures (IMF 2013).

Yet, the predominant application of macroprudential policies (MaPs) to banks may be subject to a boundary problem, causing substitution flows to less regulated parts of the financial sector. As noted by Goodhart (2008), "the more effective regulation is, the greater the incentive to find ways around it." This concern also applies to MaPs. Several papers have already estimated the intended effects of MaPs on variables such as banking credit and housing prices (e.g., Morgan, Regis, and Salike 2015, Cerutti, Claessens, and Laeven 2017, Akinci and Olmstead-Rumsey 2018), and whether measures "leak" through foreign banks (Aiyar, Calomiris, and Wieladek 2014, Reinhardt and Sowerbutts 2015, Frost, van Horen, and de Haan 2017, Cerutti and Zhou 2018). Such substitution effects across sectors have not yet been tested empirically in a cross-country setting. This paper aims to fill this gap, by estimating the effect of MaPs on nonbank credit to the nonfinancial private sector.[2] Testing the effect of MaPs on various types of credit provides information of its effects in addressing the credit cycle, but not its ultimate effect on the probability and severity of a financial crisis.

Measuring the effects of MaPs on bank credit is subject to endogeneity problems, as MaP decisions are taken in response to credit and financial cycles. Focusing on the *side effects* of MaPs, as we do, lessens these concerns, as developments in nonbank credit are unlikely to have a major influence on MaPs that apply to banks. Still, policy measures may not be completely orthogonal to developments in nonbank credit, as nonbank credit may be correlated with bank credit, and may thus influence policy decisions due to its effect on total credit. We try to address this by using generalized method of moments (GMM) estimation techniques and conditioning the effect of MaPs on nonbank credit on the effect of MaPs on bank credit. In addition, we address endogeneity by applying propensity score matching (PSM), which works in two steps, starting with explaining policy action as a left-hand side variable. Whereas the panel regressions capture the effect on nonbank credit growth after policy action, PSM compares nonbank credit growth between similar treatment (with MaPs) and control (without MaPs) groups. Another advantage of PSM is that it does not rely on a baseline specification for nonbank credit.

The boundary hypothesis predicts that more binding constraints lead to stronger substitution effects. We therefore test the substitution effects conditional on the effect on bank credit. We check whether the results differ for advanced economies (AEs) versus emerging market economies (EMEs), which differ in their degree of financial development and especially the depth of their nonbank credit markets. Moreover, next to a global database with yearly observations of MaPs, we corroborate the results with quarterly observations on prudential measures (PMs), which represent a broader

2. Total credit consists of bank loans, nonbank loans and debt securities. See Section 1 for further details.

category of policy tools including (microprudential) bank capital requirements. Finally, we test whether results differ before and after the global financial crisis (GFC).

We find indications of substitution (or "waterbed") effects in all of the methodologies and samples used, although the results differ across specifications and are subject to substantial margins of uncertainty. Results are more robust for AEs, where the size of nonbank credit is much larger than in EMEs so that there are more options for substitution toward nonbank credit. For the simple detrending method, we find that after 2 years, nonbank credit is roughly 8 percentage points (pp) above the trend following the application of MaPs in AEs, conditional on a decline in the growth rate in bank credit (as a first proxy for the degree to which MaPs were binding on bank credit).[3] Our panel regressions, which control for the impact of other variables on nonbank credit growth, suggest that substitution effects only occur in AEs and when quantity constraints are applied. Overall, during the whole period after policy measures are activated, annual nonbank credit growth is estimated to be 1.8 pp higher for MaPs and 0.5 pp higher for PMs. This is still a substantial effect relative to average yearly nonbank credit growth of 7.3% in AEs (i.e., it would rise to 9.1% on average after the activation of MaPs). Finally, when we zoom in on individual measures and match treatment and nontreatment groups using PSM, the cumulative effect on yearly nonbank credit growth is up to 9 pp above the baseline during the 2 years after activation for AEs, that is, comparable to the conditional detrending method. The higher magnitude during the first 2 years suggests that the bulk of the substitution effect is in the first years after implementation, as the long-run effect on credit growth as measured in the panel regression is smaller.

Our paper builds on a wider literature on MaP. While the concept of macroprudential policy can be traced back at least to the late 1970s (Clement 2010), it has become a common part of the policy lexicon in the first decade of this millennium. The crisis has led not only to much more interest in the macroprudential approach, but also to active use of macroprudential instruments around the world. Galati and Moessner (2013, 2017) provide an overview of the literature, emphasizing the objectives, instruments, and analytical underpinnings of the macroprudential approach. This is motivated by market failures and externalities that may lead to systemic risk, generally defined as the risk of a disruption in the financial system that is large enough to have serious negative effects on the real economy. Our focus is on MaPs that address the time dimension of systemic risk, that is, the endogenous build-up of systemic risk over the financial cycle (Borio 2014).[4]

Recently, the active use of instruments has spawned a growing empirical literature on the effectiveness of MaPs, both in individual country or regional cases and in global panels (Arregui et al., 2013). The most comprehensive approach is that of Cerutti, Claessens, and Laeven (2017), who use an IMF survey to document MaPs

3. Throughout the paper, we refer to a change in (nominal) volumes with percentages (%), and to differences between percentage changes using pp.

4. The cross-section dimension of systemic risk captures interconnections in financial networks that may transmit instability through the financial system and to the real economy.

for 119 countries over the 2000–13 period. They find that the implementation of such instruments is generally associated with the intended downward impact on credit, with most emphasis on bank credit, but that the effects are weaker in financially more developed and open economies. Bruno and Shin (2014) find that MaPs employed in Korea to deal with the effects of cross-border capital flows—such as the "macroprudential levy"—helped to reduce the sensitivity of capital flows into Korea to global conditions. Krznar and Morsink (2014) establish that recent rounds of MaP tightening in Canada have reduced mortgage credit growth and house price growth. Glocker and Towbin (2015) show that an increase in reserve requirements leads to a contraction in domestic credit, a depreciation of the exchange rate, a current account improvement, and an increase in prices. Lim et al. (2011) show that for 49 countries reviewed, MaPs helped to reduce procyclicality, meaning a reduced sensitivity of credit conditions to GDP growth. Gambacorta and Murcia (2017) use granular credit registry data and show that MaPs have been quite effective in stabilizing credit cycles in the Americas region. Altunbas, Binici, and Gambacorta (2018) demonstrate that the use of MaP reduces bank risk taking.

Because of the inherent difficulties in establishing the effects of measures at a macro level, a number of studies have used microlevel data on behavioral effects of MaPs. For example, Kim, Plosser, and Santos (2018) find that nonbanks in the U.S. increased their leveraged loan activity following the interagency guidance in 2013—and increased their bank borrowing—while large banks cut back leveraged loans. Jiménez et al. (2017) investigate bank-specific shocks and show that Spain's dynamic provisioning requirements helped to smooth cycles in the supply of credit. With Korean data on housing and mortgage activity, Igan and Kang (2011) find that the tightening of DTI and LTV limits have a significant and sizeable impact on transaction activity and house price appreciation.

Our empirical framework builds on research that has sought to explain credit growth, for instance to understand credit rationing and the monetary transmission mechanism (Gertler and Gilchrist 1991, Berger and Udell 1992, Kashyap, Stein, and Wilcox 1993). In line with Frost and van Tilburg (2014), we control for macroeconomic fundamentals to filter out the effects of policy on credit growth in a cross-country panel setting.

Our results do not tell whether substitution effects reduce or increase systemic risks. The former outcome may be expected, as risks may shift to institutions that are less leveraged, less connected to payments infrastructure, and less subject to maturity mismatch. But this need not be the case, as market failures and systemic risks may also arise outside the regulated banking sector. Specifically, nonbank financial institutions may contribute to procyclical leverage (Adrian and Shin 2009, 2010), may amplify the impact of price changes and flows (Feroli et al., 2014), and may be subject to misaligned incentives that influence the overall risk in the system (Rajan 2006).

The rest of the paper is organized as follows. Section 1 describes the data. Section 2 investigates differences from trends in credit following policy events. Section 3 presents panel regressions. Section 4 also applies a PSM that compares developments in nonbank credit following MaP activation with those in matched control groups. Section 3.2 presents robustness checks and Section 5 concludes.

1. DATA

The analysis in this paper is based on three types of country-level data: (i) information on bank and nonbank credit, (ii) the dates and types of macroprudential and broader prudential policy measures, and (iii) macrofinancial control variables.

1.1 Private Credit to the Nonfinancial Sector

Our measures of bank and nonbank credit come from the BIS long series database on private nonfinancial sector credit (Dembiermont, Drehmann, and Muksakunratana 2013). The database contains quarterly series of private credit data for 40 economies for a period covering the last 40 years. The measure of *private credit* covers all loans and debt securities to nonfinancial corporations, households, and nonprofit institutions serving households. *Bank credit* is defined as all loans and debt securities held by domestic and foreign banks domestically (subsidiaries and branches). *Nonbank credit* encompasses loans and debt securities held by all other sectors of the economy (e.g., insurers, pension funds, investment funds, other firms, households, etc.) and, for some countries, direct cross-border lending by foreign banks from abroad. The presence of direct cross-border lending in the nonbank credit measure may hamper the cross-sectoral focus of this study because it may conflate loans by domestic nonbanks and foreign banks abroad. To ensure that our measure is picking up only nonbank credit, we deduct direct cross-border lending from the BIS nonbank credit measure for those countries and quarters in which data are available. For the quarters in which data are not available, we deduct the average available share of cross-border lending, based on an average from the available quarters.[5] Overall, direct cross-border lending amounts to less than 5% of nonbank credit for the aggregate sample of BIS reporting countries in the quarters for which data are available. But this is not the case for all individual countries. We therefore remove three countries from our sample for which the amount of direct cross-border lending was larger than 50%.[6] To limit the influence of outliers, we winsorize all credit-related variables at the 1% level for each tail of their distribution. After these corrections, movements in the nonbank credit series are expected to primarily reflect changes in the provision of credit by nonbank financial institutions, rather than by foreign banks.

Panel A of Table 1 provides summary statistics on bank and nonbank credit and financial institutions' assets in AEs and EMEs for the period 1997–2014

5. With thanks to Matthias Drehmann and Stevan Advjiev (BIS) and Win Monroe (IMF) for helpful advice on these data corrections, and to Linda de Zeeuw (DNB) for compiling and correcting the necessary data from the BIS consolidated and locational banking statistics.

6. Specifically, we removed Greece, Malaysia, and Saudi Arabia from the sample. We also exclude Argentina, which experienced a prolonged sovereign distress episode covering much of our sample period.

TABLE 1

Summary Statistics on Credit, Macroprudential Policies, and Macroeconomic Indicators (Mean Values Across Subsamples and Full Sample; Standard Deviations in Parentheses)

	1997–2014, Quarterly		
	Advanced economies	Emerging market economies	Full sample
# Countries	28	9	37
# Observations	2,072	675	2,747
Panel A: Credit series			
Bank credit to private sector % of GDP, Source: BIS	84.79	60.41	77.83
	(36.86)	(42.48)	(40.08)
Nonbank credit to private sector % of GDP, Source: BIS	55.78	9.32	42.49
	(41.25)	(14.18)	(41.39)
Bank credit, y-o-y % change, Source: BIS	6.47	10.53	7.65
	(11.18)	(16.35)	(13.03)
Nonbank credit, y-o-y % change, Source: BIS	7.33	11.59	8.57
	(13.95)	(31.87)	(20.90)
Total credit, y-o-y % change, Source: BIS	6.68	9.78	7.59
	(9.98)	(15.10)	(11.79)
Panel B: Other macroeconomic variables			
Inflation, average consumer prices, Source: IMF-WEO	2.91	7.27	6.00
	(2.75)	(7.45)	(6.74)
Y-o-y real % growth in GDP, Source: IMF-IFS,	3.58	5.59	5.00
	(7.48)	(7.78)	(7.75)
Current account balance, Source: IMF-WEO	1.90	−5.03	−3.04
	(11.53)	(10.35)	(11.15)
General government net lending/borrowing, Source: IMF-WEO	−0.10	−2.14	−1.54
	(7.28)	(5.56)	(6.18)
Equity inflows, % of GDP, Source: IMF-IFS	6.17	1.43	2.79
	(11.56)	(4.81)	(7.71)
Debt inflows, % of GDP, Source: IMF-IFS	11.78	1.52	4.47
	(33.73)	(8.11)	(19.89)
Central bank policy rate (in %), Source: IMF-IFS	7.26	16.73	13.82
	(4.65)	(9.93)	(9.70)
YtY % Growth in central bank assets, Source: WB-GFDD	8.97	13.00	11.81
	(47.49)	(43.89)	(45.02)
GDP per capita, current prices, Source: IMF-WEO	29,042	2,943	10,433
	(17,691)	(2,951)	(15,342)
Banking crisis dummy	0.19	0.03	0.07
(1 = banking crisis, 0 = none): Source: Laeven and Valencia (2013)	(0.39)	(0.16)	(0.26)

(1997–2012).[7] Banks are an important source of credit in both AEs, where bank credit is 85% of GDP, and in EMEs, where bank credit is 60% of GDP. Nonbank credit, on the other hand, is much more important in AEs, comprising 56% of GDP,

7. In our data set, the group of AEs consists of the following countries: Australia, Austria, Belgium, Canada, Czech Republic, Denmark, Finland, France, Germany, Hong Kong, Ireland, Italy, Japan, Luxembourg, Netherlands, New Zealand, Norway, Poland, Portugal, Russia, Singapore, Spain, Sweden, Switzerland, the United Kingdom, and the United States. The group of EMEs consists of: Brazil, China, Hungary, India, Indonesia, Mexico, South Africa, Thailand, and Turkey.

TABLE 2

Classification of Macroprudential Policies—CCL Data Set

Abbreviation	Name	Number of events [share]	Price/quantity restriction	Price/quantity restriction (robustness)	Borrower/lender (robustness)
LTV	Loan-to-value ratio	32 [18%]	Quantity	Quantity	Borrower
DTI	Debt-to-income ratio	23 [13%]	Quantity	Quantity	Borrower
DP	Time-varying/ dynamic loan-loss provisioning	10 [5%]	Price	Price	Lender
CTC	General countercyclical capital buffer requirement	6 [3%]	Price	Price	Lender
LEV	Leverage ratio	13 [7%]	Quantity	Price	Lender
SIFI	Capital surcharges on SIFIs	7 [4%]	Price	Price	Lender
INTER	Limits on interbank exposures	16 [9%]	Quantity	Quantity	Lender
CONC	Concentration limits	22 [12%]	Quantity	Quantity	Lender
FC	Limits on foreign currency loans	15 [8%]	Quantity	Quantity	Lender
RR	Reserve requirement ratios	12 [7%]	Quantity	Price	Lender
CG	Limits on domestic currency loans	7 [4%]	Quantity	Quantity	Lender
TAX	Levy/tax on financial institutions	17 [9%]	Price	Price	Lender

TABLE 3

Classification of Prudential Measures—CCFS Data Set

Abbreviation	Name	Number of events [share]	Price/quantity restriction	Price/quantity restriction (robustness)	Borrower/lender (robustness)
LTV_CAP	Loan-to-value ratio limits	72 [13%]	Quantity	Quantity	Borrower
IBEX	Interbank exposure limits	24 [4%]	Quantity	Quantity	Lender
RR	Reserve requirements	221 [39%]	Quantity	Price	Lender
CONCRAT	Concentration limits	32 [6%]	Quantity	Quantity	Lender
SSCB	Sector-specific capital requirements	106 [19%]	Price	Price	Lender
CAP_REQ	General capital requirements	100 [18%]	Price	Price	Lender

as opposed to just 9% of GDP in EMEs. Nominal credit growth, measured by year-to-year percentage changes in the nominal stock of sectoral credit, is on average lower in AEs than in EMEs. In the former, bank and nonbank credit grew on average by 6.5% and 7.3% per year, whereas in the latter they grew by 10.5% and 11.6%, respectively. Total credit has grown by an average annual rate of 6.7% in AEs and 9.8% in EMEs.

TABLE 4

MaP and PM Events by Type and Region

	2002–8	2009–14	Total
CCL data set	73	72	145
Advanced economies	10	26	36
Quantity-based	10	13	23
Price-based	0	13	13
Emerging market economies	63	46	109
Quantity-based	51	31	82
Price-based	12	15	27
CCFS data set	186	311	497
Advanced economies	44	137	181
Quantity-based	30	48	78
Price-based	14	89	103
Emerging market economies	142	174	316
Quantity-based	101	116	217
Price-based	41	58	99

1.2 MaP Events

We use two data sources for measures of policy actions. First, we use the cross-country data set of Cerutti, Claessens, and Laeven (2017) (henceforth: CCL), who create a set of indicator variables that measure the implementation of various MaPs in 119 countries over 2000–13 at annual frequency. Their database is constructed from responses to the IMF's Global Macroprudential Policy Instruments (GMPI) survey, reported by the participating countries' financial authorities (IMF 2013). The analysis covers 12 categories of MaPs, described in Table 2.[8] Limits on foreign currency and domestic currency loans and reserve requirements have been the norm in EMEs, whereas leverage ratios and limits on interbank exposures are most frequently applied in AEs. Overall, the most popular MaPs in both AEs and EMEs are LTV limits, DTI limits, and concentration limits, the latter of which restrict the fraction of bank assets tied to a particular type of borrower.

Second, we use the cross-country database of Cerutti, Correa, Fiorentino and Segalla (2017) (henceforth: CCFS). Their focus is on PMs, taking into account both microprudential and macroprudential objectives, covering five types of prudential instruments described in Table 3. Their data set covers 64 countries over the period 2000Q1 to 2014Q4, at a quarterly frequency. Like CCL, they rely on the GMPI survey, but they also use primary sources such as central bank reports. Whereas CCL gauge the activation of measures, CCFS gauge both the tightening and loosening of measures. This leads to a much higher number of changes in the overall index.

8. CCL classify measures as financial institution-based or borrower-based. Financial institution-based policies are those aimed at financial institutions' assets or liabilities and include, for example, loan-loss provisioning practices, leverage limits, and capital buffer requirements. Borrower-based measures are those aimed at borrowers' leverage and financial positions, and cover, for example, LTV and DTI caps. Our categorization of price- versus quantity-based measures is described below.

All policy events, both MaPs and PMs, are measured as dummy variables for individual measures. The CCL database records the number of MaPs of a particular type implemented by a country at a given point in time. MaP events are defined as any increase in the number of MaPs used. The CCFS database records explicit tightening and loosening measures. We define any tightening as a PM policy event in the event study analysis, while for the panel regressions we include both tightening and loosening measures. In total, there are 145 MaPs in the CCL data set, 72% of which are quantity-based. Most events in AEs are clustered during the period 2009–14; prior to that MaPs were implemented mostly in EMEs. Meanwhile, there are 497 PM tightening events in CCFS, of which 59% are quantity-based. These events are distributed somewhat more evenly across AEs and EMEs, with more events in the postcrisis period, as can be seen in Table 4.

The CCL and CCFS data sets measure only partly overlapping sets of policy actions. The correlation coefficient between policy activation in both data sets is approximately 0.5,[9] so that differences in results are expected. Ultimately, the choice of data set depends on the question at hand. CCL focus on measures of a macroprudential nature, which matches our research question. CCFS focus on PMs, which also include measures taken for microprudential purposes, such as general bank capital requirements.

Neither database gauges the intensity of measures, as all measures are coded as dummy variables. This is disconcerting given our interest in the boundary problem, as more binding measures are expected to generate stronger substitution effects (and nonbinding measures are not expected to generate substitution effects). To address this issue to some degree, we distinguish between price- and quantity-based measures, as reported in Tables 2 and 3 for each individual measure. Examples of quantity-based measures are limits on interbank and foreign currency exposures, both of which act as a cap on the balance sheet exposures to the particular asset classes, or LTV caps, which limit the amount of a loan. These measures are designed as a direct constraint, and could therefore also be seen as a direct intervention in the market. Price-based policies include countercyclical capital buffers and dynamic provisioning requirements, which have a more indirect transmission to bank credit through their effect on the liability side of the balance sheet. The distinction between quantity and price classifications is admittedly fuzzy in some cases. For example, assuming that the supply of bank capital is constrained, we classify the leverage ratio as a quantity measure, since it effectively caps the balance sheet size of the affected entity. A leverage ratio cap could, however, also be seen as a price-based measure, since the bank could in principle expand its balance sheet by raising new capital, which would affect the average cost of funding. We therefore also apply an alternative distinction between price and quantity measures, which we use as a robustness check. Finally, we present whether measures are classified as borrower-based or lender-based.

9. This refers to the correlation between the cumulative MaP indices based on the CCL and CCFS data sets.

Inspection of the underlying qualitative answers in the IMF GMPI database indicates that the vast majority of MaPs are aimed at depository institutions (banks), including the borrower-based measures (for details see Tables 2 and 3). For example, most LTV and DTI limits only apply to bank mortgages, but not to mortgages offered by insurers, pension funds, or investment funds. The additions in the CCFS database also focus heavily on banking. Our hypotheses on substitution effects between bank and nonbank credit can therefore be tested by including all MaPs or PMs simultaneously.

1.3 Macrofinancial Control Variables

We use a number of macroeconomic and financial variables as controls. For instance, we adjust for the occurrence of systemic banking crises by including the banking crisis indicator of Laeven and Valencia (2013) as a control variable. This crisis indicator flags those country-quarter observations during which a country experienced a systemic banking crisis. Since Laeven and Valencia define banking crises with reference to the use of crisis management tools, such as deposit guarantees and government recapitalizations of failed banks, the inclusion of this indicator addresses the concern that our results on MaPs might be picking up the effects of other policies implemented during the same period of time. Moreover, we include banking system *Z*-scores, which measure the distance to default of a country's banking system, based on data from the World Bank Global Financial Development Database (GFDD). This series measures the resilience of banks, which may be an important indicator of structural systemic risk. Inflation, GDP growth, the current account balance, net government borrowing, capital inflows (debt and equity),[10] and central bank policy rates are taken from the IMF's World Economic Outlook (WEO) and International Financial Statistics (IFS) databases. Central bank assets are taken from the World Bank. Panel B of Table 1 provides summary statistics for the macroeconomic indicators used in the regressions.

2. TREND DEVIATIONS FOLLOWING POLICY ACTION

To get a first insight in developments in nonbank credit around policy events, we start by simply investigating whether growth rates differ from the trend. At this stage, we do not yet control for the impact of other variables, which we will do in Sections 3 and 3.2. Hence, we calculate the average cumulative nonbank credit growth rates during the 2-year period before and after policy events. The cumulative growth rates during the 2-year period before the event act as a baseline. They have been detrended, that is, adjusted linearly to 0% on average. Next, for the time interval between a and b periods after policy events, we compute the cumulative excess growth rate (CEGR)

10. In line with Frost and van Tilburg (2014), equity flows are defined as foreign direct investment plus portfolio equity flows, while debt flows are the sum of portfolio debt and other flows.

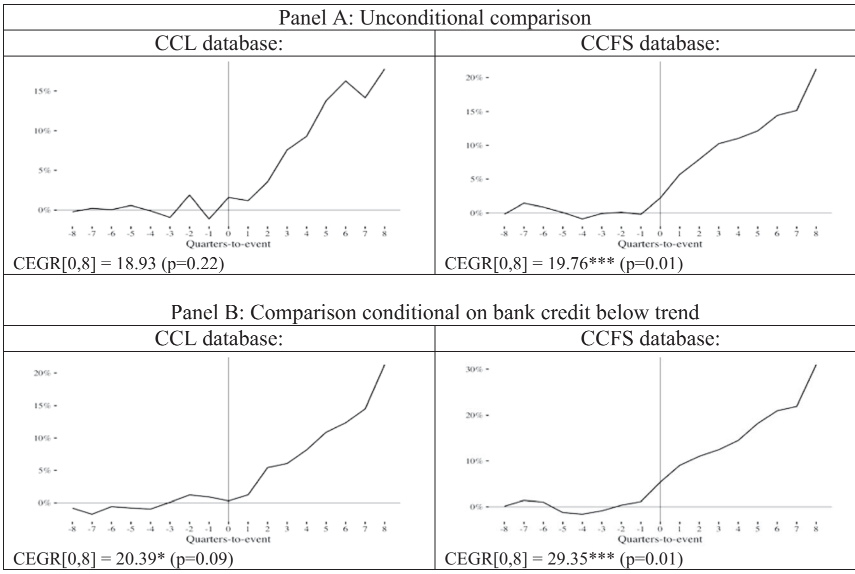

FIG. 1. Nonbank Credit Growth around Policy Events: Detrending Event Study Method.

NOTES: The figure shows the effects of MaP events on the average cumulative credit growth rates during the 2-year period following the activation of macroprudential policies. The actual postevent growth rates are adjusted by the linearly extrapolated growth rates from the 2-year preevent period. $*p < 0.1$, $**p < 0.05$, $***p < 0.01$. Standard errors in parentheses.
SOURCE: Authors' calculations.

of nonbank credit as follows:

$$CEGR^s[a, b] = \sum_{i \in [a,b]} \phi_i^s,$$

where ϕ_i measures the excessive growth in sector S of country C, i periods after the policy event. Under the null hypothesis of no differences in cumulative growth rates after policy events, the CEGR of nonbank credit is expected to be statistically indistinguishable from 0 after policy action. Conversely, to the extent that growth rates differ, CEGRs may systematically diverge from 0. We test the hypotheses related to CEGR by performing a series of Wald tests on the sums of coefficients in the specification. Moreover, we show results conditional on a negative response in bank credit following policy activation, given that the response is expected to be higher for binding measures.

Summary results for nonbank credit are shown in Figure 1, for each of the two data sets and methods. These always show higher cumulative nonbank credit growth after policy activation, totaling 18.9 and 19.8 pp for the CCL and CCFS data sets after 2 years. Next, we want to see what happens to nonbank credit when bank credit appears more constrained. We therefore measure the response in nonbank credit growth following MaPs, conditional on bank credit growth being below the preevent trend. In these circumstances, nonbank credit growth increased by 20.4 and 29.4 pp in the 2 years after MaP was activated. The results are statistically significant for both data sets under the conditional approach and for the CCFS data set under the unconditional approach.

The high numbers for cumulative nonbank credit growth following policy events may partly be due to the low level of nonbank credit in EME. Table 5 therefore shows results for AEs and EMEs separately. This confirms that cumulative growth rates in nonbank credit are lower for AEs than EMEs. Results are more robust for AEs under the conditional approach, that is, between 8 and 9 pp for both data sets. Moreover, to put the results for nonbank credit into perspective, those for total credit and bank credit are also reported. These results indicate that total credit still decreased following policy events, as the increase in nonbank credit does not offset the statistically significant, more substantial decrease in bank credit.[11]

3. PANEL REGRESSIONS

3.1 Baseline Results

The next step is to investigate the effect of MaPs on nonbank credit while controlling for the impact of other variables. Several papers have explained bank or total credit growth, including Cerutti, Claessens and Laeven (2017) who estimate the effect of MaPs. However, these studies have not explained developments in nonbank credit, as we do. The regression model is:

$$y_{c,t}^{nonbank} = y_{c,t-1}^{nonbank'}\,\alpha + MaP_{c,\,t-1}\beta + y_{c,t}^{bank}\gamma + Controls'_{c,t-1}\delta + u_c + \varepsilon_{c,t},$$

where $y_{c,t}^{s}$ denotes a vector of credit growth by sector s (i.e., nonbank or bank) in country c at time t. The lagged dependent variable captures persistence in nonbank credit growth. As before, our main variable of interest MaP is a macroprudential policy index, constructed as the cumulative sum of policy measures, where each activation adds one unit to the index and each deactivation subtracts one unit from the index.

We are interested in the substitution effect of macroprudential measures aimed at bank credit on nonbank credit. We expect this effect to be more pronounced in AEs, with greater substitution options due to the much larger size of the nonbank financial sector. We also expect substitution effects to be stronger for tighter measures, that is, those that constrain bank credit more. We use the same control variables as Cerutti, Claessens and Laeven (2017), with one difference. In our specification, we control for developments in bank credit to capture a general component of the credit cycle,

11. Note that the growth rates of nonbank and bank credit do not need to add up to the growth rate for total credit, given that the size of nonbank and bank credit relative to GDP differs substantially. Generally, the volume of nonbank credit is smaller than the volume of bank credit (and very small in EMEs), so that the higher growth rate for nonbank credit usually implies lower nominal growth in absolute terms.

TABLE 5

Credit Growth Around Policy Events: Detrending Event Study Method

Panel A: Unconditional correlations

	CCL data set			CCFS data set		
	Nonbank credit	Bank credit	Total credit	Nonbank credit	Bank credit	Total credit
All	18.93 ($p = 0.22$)	−1.59 ($p = 0.62$)	−1.53 ($p = 0.63$)	19.76*** ($p = 0.01$)	3.63* ($p = 0.06$)	2.36 ($p = 0.37$)
AEs	−3.84 ($p = 0.41$)	−4.96 ($p = 0.16$)	−4.86 ($p = 0.16$)	9.92** ($p = 0.03$)	−2.04 ($p = 0.46$)	−1.40 ($p = 0.59$)
EMEs	41.24 ($p = 0.27$)	0.26 ($p = 0.97$)	−0.58 ($p = 0.93$)	71.75*** ($p = 0.01$)	5.76 ($p = 0.33$	4.39 ($p = 0.40$)

Panel B: Correlations conditional on a negative response in bank credit

	CCL data set			CCFS data set		
	Nonbank credit	Bank credit	Total credit	Nonbank credit	Bank credit	Total credit
All	20.39* ($p = 0.09$)	−8.82** ($p = 0.03$)	−4.01 ($p = 0.25$)	29.35*** ($p = 0.01$)	−8.26*** ($p = 0.01$)	−6.17** ($p = 0.05$)
AEs	8.88*** ($p = 0.00$)	−10.11** ($p = 0.04$)	−7.78 ($p = 0.13$)	8.26** ($p = 0.04$)	−7.56** ($p = 0.03$)	−5.29 ($p = 0.11$)
EMEs	79.51 ($p = 0.42$)	−5.73 ($p = 0.39$)	−4.48 ($p = 0.51$)	130.90 ($p = 0.36$)	−15.28** ($p = 0.02$)	−14.92 ($p = 0.17$)

Notes: Standard errors in parentheses. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Source: Authors' calculations. The results represent changes in cumulative credit growth rates during the two years following policy action.

and we focus on contemporaneous substitution between bank and nonbank credit.[12] This implies that we need to leave out GDP growth, as it is tightly correlated with bank credit growth and including them both would lead to multicollinearity. The other control variables are the central bank policy rate and the occurrence of systemic banking crises. The variable u represents a country-fixed effect and ε is the error term.

Endogeneity issues may arise in case MaP reacts to the bank credit cycle. Typically, policy measures are expected during the upturn of the credit cycle, so that a subsequent decrease in credit may follow from the turning of the credit cycle. Insofar as bank and nonbank credit are positively correlated, this may also show up as a negative effect of MaPs on nonbank credit. Note, however, that such an effect would bias our results downward, while the substitution effect works upward. Moreover, as indicated, endogeneity issues appear less disconcerting than in estimating intended effects of MaPs, given that policymakers are unlikely to impose constraints on banks due to developments in nonbank credit. In any case, to alleviate issues related to reverse causality, the right-hand side variables are lagged one period (except bank credit growth, as indicated). We use the Arellano and Bond (1991) GMM estimator, which addresses the Nickel bias on the lagged dependent variable. This methodology is suitable for independent variables that are not strictly exogenous.

Results are reported in Table 6 for the two data sets, for AEs and EMEs, and for quantity- and price-based measures. In all specifications, nonbank credit growth is highly persistent, with a coefficient of 0.7–0.8. The expected positive correlation with bank credit growth is visible only for EMEs, with a coefficient of 0.2–0.3, which implies a long-run coefficient of around 1 (i.e., after the short-run dynamics have played out, e.g., $0.2/(1 - 0.8) = 1$). Hence, in EMEs, bank and nonbank credit appear to comove in the long run. But this appears not to be the case in AEs, where the coefficient on bank credit growth is either zero (CCFS data set) or negative (CCL data set) after controlling for the impact of other variables. Moreover, a banking crisis appears to limit nonbank credit growth as well in most specifications, possibly due to decreases in credit demand after a banking crisis. Our main variable of interest, that is, the index for MaPs (CCL database) or PMs (CCFS) database is statistically significant only for the most evident case of quantity based instruments in AEs. The coefficients of 0.42 and 0.11 imply a long-run effect of $0.42/(1 - 0.76) = 1.77$ and $0.11/(1 - 0.76) = 0.46$, respectively. Hence, during the period of activation of a MaP, annual nonbank credit growth would be roughly 1.8 pp higher, and during the activation of a PM, it would be roughly 0.5 pp higher. This is a substantial effect relative to average yearly nonbank credit growth of 7.3% in AEs.

12. Results are robust to including lagged bank credit growth instead of contemporaneous bank credit growth.

TABLE 6

BASELINE ARELLANO-BOND ESTIMATION RESULTS

	Dependent variable: year-on-year nonbank credit growth							
	Quantity-based MaPs				Price-based MaPs			
	AE		EME		AE		EME	
	CCL	CCFS	CCL	CCFS	CCL	CCFS	CCL	CCFS
Lagged nonbank credit growth	0.761***	0.758***	0.758***	0.763***	0.773***	0.763***	0.748***	0.746***
	(32.254)	(25.530)	(23.775)	(20.394)	(35.507)	(24.765)	(28.965)	(22.521)
MPI index	0.423**	0.112***	0.607	0.040	−0.049	0.113	1.152	−0.016
	(2.352)	(3.605)	(1.206)	(1.543)	(−0.188)	(1.219)	(1.318)	(−0.263)
Bank credit growth	−0.146***	0.022	0.282**	0.212**	−0.147***	0.054	0.255*	0.230**
	(−3.044)	(0.218)	(2.209)	(2.257)	(−3.101)	(0.575)	(1.852)	(1.971)
Bank crisis dummy	−0.452**	0.020	−1.052*	−0.788**	−0.482**	−0.035	−1.116**	−1.025**
	(−2.253)	(0.098)	(−1.769)	(−2.127)	(−2.506)	(−0.156)	(−2.299)	(−2.164)
Policy rate	0.023	−0.048	−0.001	−0.007	0.024	−0.033	−0.003	−0.008
	(1.205)	(−1.201)	(−0.068)	(−0.729)	(1.149)	(−1.027)	(−0.192)	(−0.665)
Constant	−0.006	0.135	1.232	1.691*	−0.067	0.321	1.538	1.990**
	(−0.023)	(0.383)	(1.212)	(1.956)	(−0.117)	(1.231)	(1.285)	(2.170)
Observations	909	1450	662	739	909	1450	662	739
Countries	16	25	9	10	16	25	9	10
AB AR(1) Test	0.053	0.021	0.015	0.011	0.054	0.021	0.016	0.011
AB AR(2) Test	0.861	0.329	0.268	0.256	0.966	0.332	0.297	0.250

NOTE: Standard errors in parentheses. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.
SOURCE: Cerutti, Claessens, and Laeven (2017) ("CCL"); Cerutti, Correa, Fiorentino and Segalla (2017) ("CCFS"); BIS Long Series Database on Private Non-Financial Sector Credit; IMF World Economic Outlook Database; IMF International Financial Statistics (IFS) Database; Authors' calculations.

3.2 Robustness

A variety of robustness checks are conducted for the main result of statistically significant substitution effects in the case of binding quantity constraints in AEs. A first robustness check (Table 7) concerns our classification of MaPs and PMs into quantity-based and price-based measures. An alternative measure of quantity constraints only includes measures that strictly limit exposures on the asset side of bank balance sheets (see the alternative classifications in Tables 2 and 3). In this alternative approach, we classify the leverage ratio and reserve requirement ratios as price measures, contrary to our initial intuition that these measures are relatively binding in practice. Results confirm this intuition. Substitution effects of quantity constraints are still positive, but smaller in size and statistically significant only for the CCFS database. Another specification is to focus on borrower-based measures only, that is, LTV and DTI caps. In principle, substitution effects could be more muted for these measures, given that they can be designed as measures that apply to both bank and nonbank credit. However, when we consulted the questionnaires for the CCL responses in the underlying IMF database, we saw that only Canada and the Netherlands had LTV and/or DTI rules that were explicitly cross-sectoral. Results are in line with this finding, as they show quantitatively stronger and statistically significant effects on nonbank credit for the CCL database (and positive but statistically insignificant effects on nonbank credit for the CCFS database).

A second robustness test considers the difference between the precrisis and postcrisis periods. A large share of MaPs was implemented during and after the GFC. A related concern is that our baseline results capture not only the effects of MaPs, but also a host of other factors that took place during that time period. While our previous analysis tries to control for these factors by explicitly accounting for the presence of banking crises, changes in monetary policy, and other macroeconomic fundamentals, there may be remaining omitted factors that affect credit to the private sector and are also correlated with the timing of MaPs. Another concern related to identification is that few MaPs were implemented in AEs during the period before the GFC. More generally, a distinction in precrisis and postcrisis crisis periods leaves us with two short periods spanning 2000–6 and 2008–15, so that results are subject to larger uncertainty margins. Results appear to reflect these concerns. For the postcrisis period, they still show a positive substitution effect of quantity restrictions in AEs for the CCL database but not for the CCFS database of PMs. Prior to the crisis, the effect appears to be statistically insignificant, potentially due to the low number of policy implementations in the sample.

A third robustness check involves the use of annual instead of quarterly data. Using annual data implies a more ambitious identification test, as it reduces the number of observations by a factor of 4. But it also checks whether results on substitution effects hold over longer periods of at least a year. Results in the final columns of Table 7 show that changing the frequency has a strong impact on the lagged dependent variable, as it becomes statistically insignificant. But the long-run effect for the effect of MaPs and PMs is still positive and statistically significant for the CCFS database. Moreover,

TABLE 7

ALTERNATIVE ARELLANO-BOND SPECIFICATIONS FOR QUANTITY-BASED MAPS IN AES

	Quantity-based alternative		Borrower-based		Postcrisis		Precrisis		Annual observations	
	CCL	CCFS	CCL	CCFS	CCL	CCFS	CCL	CCFS	CCL	CCFS
Lagged nonbank credit growth	0.798***	0.782***	0.785***	0.795***	0.789***	0.815***	0.773***	0.777***	−0.189	0.063
	(33.66)	(20.31)	(46,924)	(23,304)	(20.92)	(9.293)	(16.45)	(15.10)	(−1.450)	(0.438)
MPI index	0.278	0.054*	0.369*	0.058	0.508**	0.020	−0.165	0.085	2.013	0.415**
	(1.294)	(1.834)	(1,772)	(1,447)	(2.079)	(0.606)	(−0.308)	(0.599)	(1.299)	(2.315)
Bank credit growth	−0.098***	0.000	−0.132***	0,000	−0.028	−0.015	−0.397**	−0.093	−0.363	−0.121
	(−2.972)	(−0.004)	(−3.299)	(0.001)	(−0.215)	(−0.124)	(−2.493)	(−0.640)	(−1.357)	(−0.637)
Bank crisis dummy	−0.616**	−0.420	−0.468**	−0,488	−0.402	−0.376	−1.287	−1.128	−0.680	−0.203
	(−2.444)	(−1.536)	(−2.062)	(−1.264)	(−0.863)	(−0.748)	(−0.832)	(−1.088)	(−0.472)	(−0.196)
Policy rate	0.044	−0.025	0.073**	−0.022	0.066	0.012	0.001	−0.039	0.698***	0.173
	(1.227)	(−0.541)	(2,119)	(−0.400)	(0.793)	(0.122)	(0.026)	(−0.515)	(3.037)	(0.705)
Constant	0.170	0.179	−0.049	0.266	−0.245	0.104	0.458	1.346**	−2.758	−0.430
	(0.472)	(0.544)	(−0.248)	(0.894)	(−0.506)	(0.151)	(0.588)	(2.039)	(−1.506)	(−0.386)
Observations	909	1,450	831	1,450	266	423	415	661	209	333
Countries	16	25	15	25	10	16	16	25	16	25
AB AR(1) Test	0.047	0.020	0.060	0.021	0.101	0.036	0.039	0.029	0.044	0.011
AB AR(2) Test	0.898	0.404	0.834	0.362	0.507	0.236	0.672	0.428	0.356	0.319

NOTES: Standard errors in parentheses. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.
SOURCE: Cerutti, Correa, Fiorentino, and Segalla (2017) ("CCL"); Cerutti, Correa, Fiorentino and Segalla (2017) ("CCFS"); BIS Long Series Database on Private Non-Financial Sector Credit; IMF World Economic Outlook Database; IMF International Financial Statistics (IFS) Database; Authors' calculations.

TABLE 8

EXPLAINING THE IMPACT OF MACROPRUDENTIAL POLICIES AND PRUDENTIAL MEASURES: FIRST STAGE REGRESSIONS IN THE MATCHING PROCEDURE

	CCL data set				CCFS data set			
	Baseline	Baseline + cross -border	Baseline + cross -border + bank *Z*-score	Full model	Baseline	Baseline + cross -border	Baseline + cross -border + bank *Z*-score	Full model
Intercept	−13.07	−13.08	−0.97**	−1.21***	−13.27	−13.30	−2.31***	−2.43***
Bank credit growth	0.10***	0.11***	0.13***	0.11***	0.03	0.01	−0.01	−0.01
Nonbank credit growth	0.01**	0.01**	0.02**	0.02**	0.01	0.01	0.00	0.00
Cross-border credit growth		−0.02	−0.04**	−0.02		0.04**	0.05***	0.06***
Banking sector *Z* score			−0.03***	−0.04***			−0.02***	−0.02***
Bank crisis in last 10 years				0.72***				0.28**
Observations	2,436	2,436	1,689	1,689	2,974	2,974	2,120	2,120
Model L.R.	1034.8	1037.2	637.6	668.7	415.5	421.8	157.2	162.3
R^2	0.484	0.485	0.422	0.439	0.243	0.246	0.117	0.120

NOTES: Standard errors in parentheses. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.
SOURCE: Authors' calculations. The dependent variable is a dummy equal to one if a policy action is taken in the following 2 years, which is equivalent to lagging all explanatory variables to mitigate endogeneity.

the long-run effect is comparable to our baseline results, that is, 2.01/(1 + 0.19) = 1.69 pp for MaPs (versus 1.77 in the baseline regression) and 0.42/(1 – 0.06) = 0.44 pp for PMs (versus 0.46 in the baseline).

4. PSM

4.1 Methodology

The panel regressions of the previous section have at least two disadvantages. First, policy action may be endogenous to financial cycle variables, which include credit. Our argument is that this may be less of an issue for nonbank credit, as policy is expected to react primarily to bank credit. We can test this assumption by running a regression that uses policy action as a dependent instead of independent variable, with bank and nonbank credit as independent variables. Second, the event study methodology relies on a baseline regression to correct for potential confounding factors. Yet, this leaves open the possibility of omitted variable bias, particularly for nonbank credit, where the baseline regression has relatively less explanatory power. This calls for a methodology that does not rely on a baseline regression for credit, but directly compares matched observations with and without policy action.

For these reasons, we complement our event study method with a PSM approach. PSM corrects for sample selection bias due to observable differences between the treatment and comparison groups. It uses these observable variables (such as credit cycle variables, banking resilience, and recent crisis experience) to estimate propensity scores, which indicate the likelihood of policy action. The propensity scores are used to select observations for countries that took policy actions (treatment group) with similar countries and periods in which the probability of taking policy action was similar, but no policy action was taken (control group). These observations are matched and the average differences in CEGRs for the treatment and control groups can be compared directly. This method provides new evidence of first-stage regressions for MaP action, comparable to, for example, the literature on monetary or fiscal policy rules.

PSM was introduced in research on medicine (Rosenbaum and Rubin 1983) and it has since become popular in macroeconomics. It simulates the effect of a randomized experiment in nonrandom, observed data. Formally, the first stage of the analysis can be expressed as:

$$p\left(x_{c,t}\right) \equiv \Pr\left(\Omega_{c,t} = 1|x_{c,t}\right) = \Phi\left(\alpha_1 + \beta_1 x_{c,t} + \varepsilon_{c,t}\right),$$

where $p(.)$is the propensity score, defined as the probability that the dummy variable $\Omega_{c,t}$ is equal to one. This dummy variable in turn denotes policy action in country c in quarter t. The probability of policy action is estimated based on $x_{c,t}$, a vector of macroeconomic control variables. Finally, $\Phi(.)$ is the cumulative distribution function of the standard normal distribution; α_1 and β_1 are an estimated constant and coefficient; and $\varepsilon_{c,t}$ an error term.

The second stage requires a matching method for selecting nontreatment observations based on their propensity scores. We start by defining an overlapping support range for the propensity score, that is, identifying a range of propensities in which there are sufficient numbers of treatment and nontreatment observations. We then exclude treatment and control units falling outside the overlapping support range. Inside the range, we match each treatment unit to up to 10 control units.[13]

The mean difference in the outcome of interest between the treatment and control group is called the average treatment effect (ATE). In many applications of PSM, the outcome being measured is a continuous variable (such as a patient's blood pressure, or a country's GDP growth). In our application it is nonbank credit, for which we apply a leads-and-lags model (see Atanasov and Black 2016). This model is suitable for checking pretreatment and posttreatment trends relative to control groups of entities (in our case countries). Pretreatment trends that are statistically different between the treatment and control groups may be indicative of anticipation effects. Postevent trends that are statistically different between both groups correspond to treatment effects. The ATE is calculated as a CEGR for the eight quarters following policy action. Hence, the post-MaP CEGR in the treatment group measures the effect of the MaP action for treated relative to nontreated units.

In order for the PSM estimation to yield unbiased estimates, it is important that the first stage involves a strong enough goodness of fit that observations can be accurately matched with similar observations, but not so strong as to perfectly divide the group into treated observations with high probability of treatment and nontreated observations with low probability. In other words, there must be enough overlap between the treatment and control groups that matching of observations is possible.

4.2 Results

The first stage can be estimated with a simple probit regression that takes our dummies on policy action as the dependent variable. The objectives of MaPs include addressing the credit cycle (i.e., the time dimension of systemic risk) and increasing the resilience of the financial system (i.e., the cross-section dimension). For the time dimension, we use growth in bank and nonbank credit, as well as cross-border credit. For the cross-section dimension, we use banking sector *Z*-scores, that is, the distance to default of a country's banking system, calculated as a weighted average of the *Z*-scores of the country's individual banks.[14] Individual *Z*-scores compare a bank's buffers (capitalization and returns) with the volatility of its returns. Finally, we include a political economy factor that may influence policy action. This relates to our observation that MaPs only became politically feasible in many parts of the world after the economic and social costs of financial crises were actually experienced by

13. The results are relatively insensitive to the number of matched control units.

14. Banking system *Z*-scores are taken from GFDD. This series measures the resilience of banks, which is an indicator of structural systemic risk. While the overall resilience of the banking system may depend on other factors than the resilience of individual banks, such as interdependencies within the financial network, the authors consider this a valuable proxy.

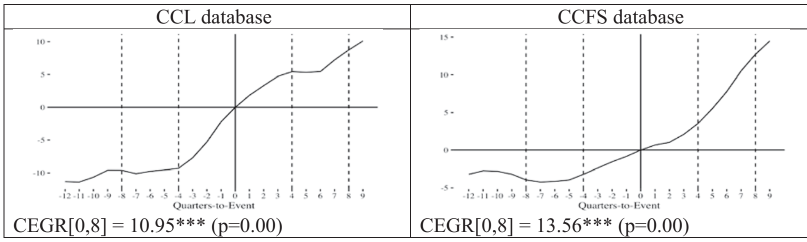

FIG. 2. Nonbank Credit Growth around Macroprudential Policy Measures: Propensity Score Matching (PSM) Method.

NOTES: The figure shows the effects of MaP events on the average cumulative credit growth rates during the period before and after the activation of macroprudential policies. The actual postevent growth rates are adjusted by the linearly extrapolated growth rates from the 2-year preevent period. $*p < 0.1$, $**p < 0.05$, $***p < 0.01$. Standard errors in parentheses.
SOURCE: Authors' calculations.

country populations. This applies to the Latin American and Asian financial crises of the 1990s and the GFC of 2007–8. To capture this effect, we include a dummy variable that signals whether a country has experienced a banking crisis in the past 10 years. As with our panel regression, endogeneity concerns are relevant also here. Policy action is expected to influence future credit growth, the stability of the banking system, and the probability of a financial crisis. To mitigate these concerns, we use a policy variable equal to one if a policy is taken in the following 2 years, which is equivalent to lagging all of our explanatory variables.

Results as reported in Table 8 confirm our expectations. Bank credit is highly relevant for the MaPs of the CCL database but not for PMs, which often have a more institution-specific, microprudential character. Nonbank credit is statistically significant in the CCL database, but has a very small economic magnitude (about 10–20% of the size of bank credit growth). It is not statistically different from zero in the CCFS database. Finally, resilience as measured by *Z*-scores and political economy factors as measured by banking crises also matter for both data sets in the expected manner. Hence, we use the regression that includes all these variables for calculating the propensity scores. Treatment observations (i.e., country-quarters where a policy measure was taken) with propensity scores close to 1, and nontreatment observations with scores close to 0, are not matched, as there are insufficient comparable observations in the other group. But in between these two clusters there are ample data that have similar propensity scores for treated and untreated units.

In the second stage, we analyze the effect of policy action by calculating the average CEGR of the treatment group relative to the control group. Figure 2 plots the CEGRs for nonbank credit for both data sets. As in the event study, the trajectory of CEGR in nonbank credit shows a statistically significant effect: during the 2 years following policy events, the growth rate of nonbank credit is 11.0 and 13.6 pp above the baseline for the CCL and CCFS databases, respectively. This finding is statistically significant with a *p*-value below 1%. It is also substantially lower than the result for the event study without control variables (i.e., 18.9 and 19.8 pp, respectively), thus confirming

TABLE 9

CREDIT GROWTH AROUND POLICY EVENTS: PROPENSITY SCORE MATCHING (PSM) METHOD

	CCL database			CCFS database		
	Nonbank credit	Bank credit	Total credit	Nonbank credit	Bank credit	Total credit
All	10.95^{***} ($p = 0.00$)	-2.18 ($p = 0.30$)	1.79 ($p = 0.24$)	13.56^{***} ($p = 0.00$)	-2.15 ($p = 0.15$)	-1.46 ($p = 0.27$)
AEs	1.80 ($p = 0.37$)	-5.50^{***} ($p = 0.00$)	-2.85^{*} ($p = 0.05$)	8.70^{***} ($p = 0.00$)	-6.29^{***} ($p = 0.00$)	-5.97^{***} ($p = 0.00$)
EMEs	27.08^{***} ($p = 0.00$)	-13.63^{***} ($p = 0.00$)	-9.19^{**} ($p = 0.01$)	49.51^{***} ($p = 0.00$)	-28.26 ($p = 0.25$)	6.58 ($p = 0.61$)

NOTES: Standard errors in parentheses. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.
SOURCE: Authors' calculations. The results represent changes in cumulative credit growth rates during the 2 years following policy action.

TABLE 10

CREDIT GROWTH AROUND POLICY EVENTS: PROPENSITY SCORE MATCHING (PSM) METHOD, BY INSTRUMENT TYPE

	Quantity measures			Price measures		
	Nonbank credit	Bank credit	Total credit	Nonbank credit	Bank credit	Total credit
CCL	14.07^{***} ($p = 0.00$)	-5.39^{**} ($p = 0.02$)	-1.39 ($p = 0.52$)	9.95^{***} ($p = 0.00$)	-1.57 ($p = 0.55$)	-0.99 ($p = 0.66$)
CCFS	13.69^{***} ($p = 0.00$)	-6.09^{***} ($p = 0.00$)	-3.07^{*} ($p = 0.07$)	13.12^{***} ($p = 0.00$)	3.96^{***} ($p = 0.01$)	5.07^{***} ($p = 0.01$)

Notes: Standard errors in parentheses. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.
SOURCE: Authors' calculations. The results represent changes in cumulative credit growth rates during the 2 years following policy action.

the value added of PSM. Moreover, there seems to be some movement before the formal activation of policies. This could relate to an anticipation in the market of future movements or, in the case of CCL data, it could relate to measurement issues around the exact timing of policies within a given year.

Table 9 puts these results for nonbank credit into perspective by distinguishing between AEs and EMEs, and comparing them to the results for bank and total credit. As expected, and in line with previous results, effects on cumulative credit growth are again higher for EMEs than for AEs. While results differ across specifications, the statistically significant results point to a positive effect on nonbank credit, a negative effect on bank credit, and a negative effect on overall credit. For AEs, we find that for the CCL and CCFS databases the cumulative effect is an increase in nonbank credit growth between 1.8 and 8.7 pp, a decrease in bank credit growth between 5.5 and 6.3 pp, and a decrease in total credit growth between 2.9 and 6.0 pp.

Table 10 shows the distinction between quantity constraints and price measures. For quantity constraints, the effect on bank credit is negative, substitution effects are positive, and the overall effect on total credit is negative. For price measures, results for nonbank credit are broadly in line with those for quantity measures, but results for bank credit are mixed, as the effect on bank credit is no longer significant (and even positive in the CCFS database, which is difficult to explain). At the same time, nonbank credit still increases.

5. CONCLUSION

MaPs are being activated in AEs and EMEs both to increase the resilience of the financial system and to dampen the financial cycle. Using a battery of empirical techniques, this paper investigates whether MaPs directed at banks lead to opposite effects on nonbank credit. Such waterbed effects are expected only when measures are binding on bank credit, and are expected to be stronger in financial systems in which there are more options to shift credit provision to the nonbank sector. Properly identifying such effects is however challenging. Existing measures of policy action do not indicate the extent to which measures are binding and endogeneity may be an issue. To address these identification challenges, we have focused on effects on nonbank credit conditional on a decline in bank credit, and on quantity constraints that are expected to be more binding and therefore produce stronger substitution effects. Moreover, to check the robustness of the results, we have used two databases of policy measures and applied three different methodologies, ranging from trend deviations to panel regressions and PSM.

Overall, our results indicate substantial substitution effects after MaP measures are implemented. The results are strongest for trend deviations conditional on a decline in bank credit. After applying a range of control variables, the findings remain statistically significant for quantity constraints in AEs. Moreover, the substitution effects are clearly visible in a comparison of similar country cases with, and without, macroprudential measures.

But the results also differ across samples and methodologies and are subject to large degrees of uncertainty. Clearly, more research is needed to gain more precise answers. In this context, there is a need for improved data availability and additional analysis of the effects of MaP on different forms of credit. For example, nonbank credit data could be improved by filtering out direct cross-border lending by foreign banks in the BIS database. Future research could address endogeneity issues by using microdata on bank and nonbank credit. Finally, research could benefit from more granular measurement of macroprudential and prudential policy measures, in particular the degree to which a specific measure is binding.

These results raise questions for policymakers on the optimal scope of MaP. On the one hand, it could be argued that substitution effects engender new systemic risks. When credit growth shifts away from banks, but households and corporates continue to accumulate debt, macroeconomic vulnerabilities may continue to rise. This may eventually prompt a crisis, even if the debt is owed to investment funds or to capital markets. On the other hand, it could be argued that cross-sector substitution reduces systemic risks. Nonbank financial institutions are generally less leveraged and have less liquidity risks than the banking sector; they are also separated from systemic functions related to the payments infrastructure. Moreover, the nonbank financial sector generally does not have access to public sector safety nets, such as deposit insurance and central bank liquidity support. Moral hazard concerns may thus be lower. In this light, policymakers may welcome a shift to market-based financing, which can function as a "spare tire" in the supply of credit in times of systemic banking crises (IMF 2015). In fact, these considerations underlie the proposals for the creation of a European Capital Markets Union (European Commission 2015). While our research cannot settle this policy debate, our results suggest substitution effects need to be taken into account in the design of MaPs.

LITERATURE CITED

Adrian, Tobias, and Hyun Song Shin. (2009) "Money, Liquidity and Monetary Policy." *American Economic Review: Papers & Proceedings*, 99, 600–5.

Adrian, Tobias, and Hyun Song Shin. (2010) "Liquidity and Leverage." *Journal of Financial Intermediation*, 19, 418–43.

Aiyar, Shekhar, Charles W. Calomiris, and Tomasz Wieladek. (2014) "Does Macro-Prudential Regulation Leak? Evidence from a UK Policy Experiment." *Journal of Money, Credit and Banking*, 46, 181–214.

Akinci, Ozge, and Jane Olmstead-Rumsey. (2018) "How Effective Are Macroprudential Policies? An Empirical Investigation." *Journal of Financial Intermediation*, 33, 33–57.

Altunbas, Yener, Mahir Binici, and Leonardo Gambacorta. (2018) "Macroprudential Policy and Bank Risk." *Journal of International Money and Finance*, 81, 203–20.

Arregui, Nicolas, Jaromír Beneš, Ivo Krznar, Srobona Mitra, and Andre Oliveira Santos. (2013) "Evaluating the Net Benefits of Macroprudential Policy: A Cookbook." IMF Working Paper 13/167.

Arellano, Manuel, and Stephen Bond. (1991) "Some Tests of Specification for Panel Data: Monte Carlo Evidence and an Application to Employment Equations." *Review of Economic Studies*, 58, 277–97.

Atanasov, Vladimir, and Bernard Black. (2016) "Shock-Based Causal Inference in Corporate Finance and Accounting Research." *Critical Finance Review*, 5, 207–304.

Berger, Allen, and Gregory F. Udell. (1992) "Some Evidence on the Empirical Significance of Credit Rationing." *Journal of Political Economy*, 100, 1047–77.

Borio, Claudio. (2014) "The Financial Cycle and Macroeconomics." *Journal of Banking and Finance*, 45,182–98.

Bruno, Valentina, and Hyun Song Shin. (2014) "Assessing Macroprudential Policies: Case of South Korea." *The Scandinavian Journal of Economics*, 116, 128–57.

Cerutti, Eugenio, Ricardo Correa, Elisabetta Fiorentino, and Esther Segalla. (2017) "Changes in Prudential Policy Instruments - A New Cross-Country Database." *International Journal of Central Banking*, 13, 477–503.

Cerutti, Eugenio, Stijn Claessens, and Luc Laeven. (2017) "The Use and Effectiveness of Macroprudential Policies." *Journal of Financial Stability*, 28, 203–24.

Cerutti, Eugenio, and Haonan Zhou. (2018) "Cross-border Banking and the Circumvention of Macroprudential and Capital Control Measures." IMF Working Paper No. 18/217.

Clement, Piet. (2010) "The Term 'Macroprudential': Origins and Evolution." *BIS Quarterly Review*, March.

Dembiermont, Christian, Mathias Drehmann, and Siriporn Muksakunratana. (2013) "How Much Does the Private Sector Really Borrow? A New Database for Total Credit to the Private Non-Financial Sector." *BIS Quarterly Review*, March.

European Commission. (2015) "Action Plan on Building a Capital Markets Union." Brussels.

European Systemic Risk Board (ESRB). (2016) "A Review of Macro-prudential Policy in the EU in 2015." May.

European Systemic Risk Board (ESRB). (2018) "A Review of Macro-prudential Policy in the EU in 2017." April.

Feroli, Michael, Anil Kashyap, Kermit Schoenholtz, and Hyun Song Shin. (2014) "Market Tantrums and Monetary Policy." Chicago Booth Research Paper No. 14-09.

Frost, Jon, and Ruben van Tilburg. (2014) "Financial Globalization or Great Financial Expansion? The Impact of Capital Flows on Credit and Banking Crises." DNB Working Paper No. 441.

Frost, Jon, Neeltje van Horen, and Jakob de Haan. (2017) "International Banking and Cross-Border Effects of Regulation: Lessons from the Netherlands." *International Journal of Central Banking*, 13, 293–313.

Galati, Gabriele, and Richhild Moessner. (2013) "Macroprudential Policy – A Literature Review." *Journal of Economic Surveys*, 27, 846–78.

Galati, Gabriele, and Richhild Moessner. (2017) "What Do We Know about the Effects of Macroprudential Policy?" *Economica*, 85, 735–70.

Gambacorta, Leonardo, and Andrés Murcia. (2017) "The Impact of Macroprudential Policies and the Interaction with Monetary Policy: An Empirical Analysis using Credit Registry Data." BIS Working Papers No. 636.

Gertler, Mark, and Simon Gilchrist. (1991) "Monetary Policy, Business Cycles and the Behavior of Small Manufacturing Firms." NBER Working Paper No. 3892.

Glocker, Christian, and Pascal Towbin. (2015) "Reserve Requirements as a Macroprudential Instrument - Empirical Evidence from Brazil." *Journal of Macroeconomics*, 44, 158–76.

Goodhart, Charles. (2008) "The Boundary Problem in Financial Regulation." *National Institute Economic Review*, 206, 48–55, https://doi.org/10.1177/0027950108099842.

Igan, Deniz, and Heedon Kang. (2011) "Do Loan-to-Value and Debt-to-Income Limits Work? Evidence from Korea." IMF Working Paper.

International Monetary Fund (IMF). (2013) "Global Macroprudential Policy Instruments Database."

International Monetary Fund (IMF). (2015) "The Asset Management Industry and Financial Stability." Global Financial Stability Report, April.

Jiménez, Gabriel, Steven Ongena, José-Luis Peydró, and Jesús Saurina. (2017) "Macroprudential Policy, Countercyclical Bank Capital Buffers and Credit Supply: Evidence from the Spanish Dynamic Provisioning Experiments." *Journal of Political Economy*, 125, 2126–77.

Kashyap, Anil K., Jeremy C. Stein, and David W. Wilcox. (1993) "Monetary Policy and Credit Conditions: Evidence from the Composition of External Finance." *American Economic Review*, 83, 78–93.

Kim, Sooji, Matthew C. Plosser, and João A. C. Santos. (2018) "Macroprudential Policy and the Revolving Door of Risk: Lessons from Leveraged Lending Guidance." *Journal of Financial Intermediation*, 34, 17–31.

Krznar, Ivo, and James Morsink. (2014) "With Great Power Comes Great Responsibility: Macroprudential Tools at Work in Canada." IMF Working Paper No. 14/83.

Laeven, Luc, and Fabián Valencia. (2013) "Systemic Banking Crises Database." *IMF Economic Review*, 61, 225–70. https://doi.org/10.1057/imfer.2013.12.

Lim, Cheng Hoon, Francesco Columba, Alejo Costa, Piyabha Kongsamut, Akira Otani, Mustafa Saiyid, Torsten Wezel, and Xiaoyong Wu. (2011) "Macroprudential Policy: What Instruments and How to Use Them? Lessons from Country Experiences." IMF Working Paper No. 11/238.

Morgan, Peter J., Paulo José Regis, and Nimesh Salike. (2015) "Loan-to-Value Policy as a Macroprudential Tool: The Case of Residential Mortgage Loans in Asia." ADBI Working Paper No. 528. Tokyo: Asian Development Bank Institute.

Rajan, Raghuram. (2006) "Has Finance Made the World Riskier?" *European Financial Management*, 12, 499–533.

Reinhardt, Dennis, and Rhiannon Sowerbutts. (2015) "Regulatory Arbitrage in Action: Evidence from Banking Flows and Macroprudential Policy." Bank of England Working Paper No. 546.

Rosenbaum, Paul, and Donald B. Rubin. (1983) "The Central Role of the Propensity Score in Observational Studies for Causal Effects." *Biometrika*, 70, 41–55.